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Work-related training and the probability of transitioning from non-permanent to permanent employment

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Abstract

It is widely believed that work-related training increases a worker's probability of moving up the job-quality ladder. This is usually couched in terms of effects on wages, but it has also been argued that training increases the probability of moving from non-permanent forms of employment to more permanent employment. This hypothesis is tested using nationally representative panel data for Australia, a country where the incidence of non-permanent employment, and especially casual employment, is high by international standards. While a positive association between participation in work-related training and the subsequent probability of moving from either casual or fixed-term contract employment to permanent employment is observed among men, this is shown to be driven not by a causal impact of training on transitions but by differences between those who do and do not receive training; i.e., selection bias.

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1 Introduction

Non-standard, contingent forms of employment, and especially temporary and casual employment – henceforth non-permanent employment – are typically equated with poor job quality (e.g., Kalleberg *et al*., 2000; McGovern *et al.*, 2004; Neinhüser and Matiaske, 2006; Dekker and van der Veen, 2015). Nevertheless, such jobs could still be welfare enhancing if they improve the chances of workers finding more stable and secure employment in the future. The evidence on this is mixed, with some research suggesting that temporary jobs (and other forms of non-standard employment) often serve as entry ports into more permanent jobs (e.g., Segal and Sullivan, 1997; Booth *et al*., 2002; Gash, 2008; Ichino *et al*., 2008; Buddelmeyer and Wooden, 2011; de Graaf-zijl *et al*., 2011), while others conclude that the rate of transition between temporary and permanent jobs can be quite low (e.g., Amuedo-Dorantes, 2000; Güell and Petrongolo, 2007), that the risk of experiencing recurrent unemployment in the future is much higher among temporary job holders (e.g., Mooi-Reci and Dekker, 2015), and that in some situations, individuals might be better off waiting for a better more secure job than accepting the first non-permanent employment opportunity (e.g., Barbieri and Scherer, 2009; Yu 2012).

Somewhat surprisingly, this growing body of evidence tells us relatively little about the type of working conditions that might be most supportive of, or harmful to, progression from a less secure non-permanent job into a more permanent job. Previous research typically controls for an extensive range of individual characteristics (such as age, sex, educational attainment, work experience and the like), but job characteristics, when considered, are usually limited to industry, occupation, working hours, and in some cases firm size. Only rarely has any serious consideration been given to the potential role that work-related training might play.

Identifying and quantifying the role that work-related training plays in assisting workers progress from non-permanent to permanent forms of employment is the aim of this study. More specifically, we use longitudinal survey data, tracking members of a nationally representative sample of households in Australia over the period 2003 to 2013, to test whether participation in a structured formal training program as part of one's employment has any positive influence on the likelihood of workers employed on non-permanent employment contracts (defined here as either fixed-term or casual employment) subsequently progressing to a more secure job providing ongoing or 'permanent' employment.

A key, and novel, feature of our analysis is that we seek to address the endogeneity of training participation due to non-random selection into training by exploiting the longitudinal nature of the data to estimate a multinomial logit model of transitions out of non-permanent employment that includes individual-specific fixed effects. The resulting evidence suggests that any positive association between participation in work-related training and transitions out of non-permanent employment is driven not by a causal impact of training on transitions but by selection bias.

2 Training and job quality

A basic assumption of human capital theory is that training increases worker productivity. This may, in turn, lead to higher wages and/or improvements in other aspects of job quality for the trained worker in the future, depending in part on the extent to which the training is general or firm-specific. Empirical evidence tends to support the prediction of a positive association between training and subsequent wages (e.g., Booth, 1993; Parent, 1999; Frazis and Lowenstein, 2005), though the magnitude of this association is generally found to be much smaller in models that seek to account for differences in unobservables between those who do and do not receive training (e.g., Booth, 1993; Leuven and Oosterbeek, 2008; Albert *et al*., 2010).

Our interest here is not in the wage effects of training, however, but in its impact on the probability of transitioning from non-permanent employment to more secure forms of employment. This raises a number of additional issues that, taken together, make for more ambiguity *ex ante*. First, because the incentives for firms and workers to invest in training depend on the time period over which the benefits of training can be realised, persons in temporary and casual jobs will be both less attractive training propositions to employers and more reluctant to participate in training than persons employed on a more permanent basis. Existing evidence is generally consistent with this (e.g., Booth *et al*., 2002; Draca and Green, 2004; Neinhüser and Matiaske, 2006). One implication of this is that non-permanent employees who do engage in training (and their employers) are likely to have different unobservables compared to those who do not, although it is unclear how this might impact on the probability of transitioning from non-permanent to permanent employment.

Second, the nature of training may be different for workers employed under different employment contract types. There is some Australian evidence, for example, that casual workers who undertake training engage in fewer hours of training than other workers (VandenHeuvel and Wooden, 1999; Draca and Green, 2004). This likely reduces the magnitude of any impact from training on transitions and on wage growth. It is also possible that the type (e.g., general versus specific) and/or quality of training differs across employment types, although there is scant evidence either way in this respect.

Third, if returns to training are heterogeneous, then systematic differences between temporary and permanent workers, or between temporary and permanent jobs, may have implications for training impacts. In particular, if low-skilled workers or those in low-wage jobs have lower returns to education than other workers (Frazis and Lowenstein, 2005; Pavlopoulos *et al*., 2009), then temporary workers, who tend to be concentrated in low-skilled, low-wage jobs, may also have lower returns to training.

Fourth, transitions from temporary to permanent employment occur within firms as well as between firms (e.g., Green and Leeves, 2004), and the impact of training on the probability of transition between employment types will depend in part on the interaction between the proportion of such transitions that occur within firms and the proportion of training that is general rather than firm-specific. If training is predominantly firm-specific but transitions to permanent employment mainly occur between rather than within firms, for example, then training, by reducing the portability of skills, might even reduce the rate of progression into permanent employment.

Given this theoretical ambiguity, credible empirical evidence of training impacts on the probability of transition from temporary to permanent employment takes on added importance. Very few studies, however, have examined this issue explicitly. Higuchi (2013), who examined the impact on firm-provided training on the probability of transition from 'non-regular' to 'regular' (permanent, full-time) employment using data drawn from the Japanese Keio Household Panel Study, is an exception. A propensity score matching approach was used to address non-random selection into training, and the resulting estimates suggested a positive association for men between firm-provided training and the subsequent probability of moving to a regular job (whether within the firm or with another firm), but no clear association for women. Interpreting these estimates as causal, however, requires a strong (conditional independence) assumption that those who undertake training and those who do not do not differ systematically in terms of unobservables.

Other than that, there have been a small number of studies within the wider empirical literature on transitions out of temporary employment that have included a measure of training as a control variable (D'Addio and Rosholm, 2005; Debels, 2008; Gash, 2008; Yu, 2012), but in none did the coefficient on this variable receive any detailed scrutiny, and in two (Gash, 2008; Yu, 2012) the variable measured the receipt of training over some undefined but long period and hence strong associations with job transitions would not be expected. In contrast, D'Addio and Rosholm (2005) and Debels (2008), both of whom analysed data from the European Community Household Panel, employed a measure that identified receipt of training in the 12 months prior to interview. While the results on this variable were not discussed, they both reported a significant positive association between participation in training by men (but not women) and the probability of subsequently transitioning (during the following year) into permanent employment. D'Addio and Rosholm (2005), however, also reported that this coefficient became statistically insignificant once unobserved heterogeneity was accounted for, though this appears to mainly reflect an increase in the imprecision of the estimate rather than a reduction in its magnitude. (There are, however, issues with their specification of unobserved heterogeneity; we return to this in Section 4.)

3. Data

The HILDA Survey

We use data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey, a longitudinal study that has been following members of a nationally representative sample of Australians households since 2001 (see Watson and Wooden, 2012). The initial sample of respondents comprised 13,969 individuals, aged 15 years or older, from 7682 households. A population refreshment sample was introduced in wave 11 (i.e., 2011), which added a further 2153 responding households. Sample sizes vary each survey wave reflecting, on the one hand, deaths and non-response, and on the other hand, changes in household composition (interviews are sought with all persons aged 15 years or older who are co-residing with an original sample member). Relatively high re-interview rates (which reached 94% in wave 5 and have remained above that level ever since), however, has meant that after declining in the initial years, the overall sample size has been growing – rising from 12,408 respondents in wave 4 to 13,526 respondents by wave 10. With the addition of the refreshment sample, the total number of survey respondents rose to 17,612 in wave 11.

For this analysis, data from waves 3 to 13 (i.e., 2003 to 2013) are used; data from waves 1 and 2 are not used given the question on work-related training, which is central to this study, was only introduced in wave 3. We restrict our sample to what is conventionally defined as the working-age population – persons between 15 and 64 years of age. This provides an initial unbalanced sample comprising 130,692 observations from 22,676 individuals. However, for our main multivariate analysis the sample is further restricted to persons employed either on a fixed-term contract or on a casual basis in their main job at the time of interview, and whose labour market status is also observed at the next interview. This reduces the usable sample to a maximum of 21,236 observations from 8,474 individuals. We also test whether our results are robust to alternative sample definitions; notably after further excluding all persons in full-time education, after restricting to prime age workers, and after restricting the sample to only those employed on a casual basis.

Among other things, the survey collects extensive information about the jobs held at the time of each annual interview, including on the nature of the employment contract.

This allows us to classify workers into different employment types. The survey also collects information on work-related training undertaken in the previous 12 months and on individual and household socio-economic circumstances and characteristics.

The HILDA Survey is not without its limitations. Of most relevance for this study, where a job transition and work-related training both occur between two given interviews, we do not know whether the training precedes the transition or vice versa. To ensure training episodes always occur prior to any observed change in employment type, and like D'Addio and Rosholm (2005) and Debels (2008), we focus on the association between work-related training that is undertaken during a 12-month period prior to interview and job transitions that occur between that interview and the next. As a result, those training events that occur closest to the date of any job transition may be missed, which could potentially attenuate our estimated training effects. We also cannot entirely rule out the possibility that in some cases training observed at time t and transitions observed at time $t+1$ may be co-determined at time $t-1$; e.g., where a promotion to a permanent job within a firm is offered conditional on undertaking training.

Classifying employment type

All respondents are initially classified into six mutually exclusive and exhaustive labour market categories. Using the standard International Labour Organization convention, the sample is first divided into three groups based on their labour force status: the employed; the unemployed; and those not in the labour force. All survey respondents who were employed at any time during the seven days prior to interview are asked whether, in their main job, they worked for an employer for wages or salary (employees), in their own business (the self-employed), or without pay in a family business. Employees are then asked to choose one among four categories that best describes their current contract of employment in their main job. The options are: (i) employed on a permanent or ongoing basis; (ii) employed on a fixed-term contract; (iii) employed on a casual basis; or (iv) employed under some other arrangement (for example, persons remunerated on a commission basis). We use the answers to (i)-(iii) to define permanent, fixed-term and casual employment respectively. The self-employed, those working unpaid in a family business, and employees in category (iv) above are lumped together into a catch-all 'other employed' category.

It is the categories of fixed-term contract and casual employment that are most commonly associated with non-standard employment in Australia and which are the focus of this analysis (what we have called non-permanent employment). Fixed-term contract workers, by definition, have no guarantee of ongoing employment beyond the duration of the current contract, while the defining feature of most casual employment arrangements is that employment can be terminated without notice.

Summary statistics describing the distribution of the working-age population by labour market state for the period covered by our data are provided in Table 1. On average, almost 14% of the Australian working-age population (or around 22% of all employees) were employed on a casual basis, and a further 6% (or 9% of employees) were employed on a fixed-term contract. The incidence of casual employment was particularly pronounced among young people (30.8% of all persons aged under 25 years) and was more common among women than men (15.7% vs 11.6%). By international standards, such proportions are high. According to the OECD statistics website (http://stats.oecd.org), for example, permanent employment in Europe has, over this same period, averaged around 88 to 89% among employees aged 25 to 54. The most comparable estimate from the HILDA Survey is just 76%. The only Western European nation with a lower incidence of permanent employment was Spain.[1]

Following Buddelmeyer and Wooden (2011), we also report, in Table 2, the average year-to-year transitions in labour market states for all persons aged between 15 and 64 years. As found by Buddelmeyer and Wooden (2011), the majority of casual employees (55.6%) are still in casual employment a year later. Nevertheless, a sizeable minority (almost 21%) will be working as permanent employees. Among persons in fixed-term contract jobs the rate of progression into permanent employment is much higher – almost 46% will be in permanent jobs a year later. Using information on tenure with the current employer to generate a measure of whether transitions occur within firms or between firms, we find that just over half (55%) of the transitions we observe from casual or fixed-term employment into a different employment type occur between firms (with the remainder obviously occurring within firms).

Identifying participation in training

The training measure used in this study is a binary variable indicating whether the respondent took part in any education or training schemes, as part of their employment during the 12 months preceding interview, with the aim of enhancing skills. The focus on training that is intended to enhance skills sets our analysis apart from all previous research in this space, and is potentially important given training can often have other purposes (e.g., to meet occupational health and safety requirements). To identify the aims of training, respondents were confronted with a set of seven options. Our training dummy takes a value of 1– we consider training to be skill-enhancing if the respondent answered yes to one or more of the following options: "to improve your skills in your current job"; "to prepare you for a job you might do in the future or to facilitate promotion"; and "to develop your skills generally", and zero otherwise.[2] The scope of training is restricted to structured training courses, and thus excludes informal on-the-job learning. The training could also relate to any job held during the 12-month reference period, and not just the main job held at the time of interview. Note there is no clear way of distinguishing the extent to which such training is general versus firm-specific, and although the first option could be interpreted as more firm-specific than the second option, in practice all three options are likely to capture elements of both types of training. Respondents also tend to select more than one option.

Table 3 reports statistics describing the incidence of work-related training over a 12-month period by employment type, sex and age group for employees of working age. Overall, around 33% of all employees (aged between 15 and 64) participate in some form of structured work-related training each year with the aim of enhancing skills. Most importantly, and consistent with both *a priori* expectations and previous research using different Australian data sources (e.g., Baker and Wooden, 1992; Miller, 1994; VandenHeuvel and Wooden, 1999; Draca and Green, 2004; Richardson and Law, 2009), the rate of training participation among casual employees is less than half that among permanent employees (17.5% compared with 36.8%). In contrast, the prevalence of training among fixed-term contract employees is no lower than that among permanent employees; indeed, it is higher. This latter finding might seem unexpected, but is entirely consistent with other Australian evidence showing that fixed-term contract workers are concentrated in high-status, white-collar occupations, and hence quite distinct from casual employees (Hall *et al*., 1998; Wooden, 2001), and that, on average, there are no significant differences in the overall job satisfaction of fixed-term contract employees and permanent employees (Green *et al*., 2010; Buddelmeyer *et al*., 2015). Also note that average training hours for those who report having received training vary by contract type in a similar fashion, consistent with the earlier findings of VandenHeuvel and Wooden (1999) and Draca and Green (2004). Specifically, casual men (women) report an average of 38 (32) hours training compared to permanent men (women) who report an average of 65 (44) hours. In contrast, fixed-term contract workers who receive training report more training hours on average than permanent workers (94 for men and 50 for women).

4. Method: modelling employment transitions

Like most previous research that has analysed exits out of non-standard employment, we utilise a multinomial logit (MNL) model, which is appropriate when the dependent variable involves a set of more than two discrete choices or outcomes.[3] The sample is restricted to employees employed on either a fixed-term contract or on a casual basis, and models are estimated separately for men and women. The outcome variable is the individual's labour market state at time *t+1*, which is specified here to comprise five categories: permanent employment; fixed-term contract employment; casual employment; other employment; and non-employment.[4] The explanatory variables in this model are participation in training, which covers the 12-month period prior to *t*, and a set of controls for observable personal and job characteristics (such as age, marital status, education level, job tenure, occupational status, and industry) which we expect to be associated with transitions out of non-permanent employment. The list of all control variables included, together with definitions and summary statistics, is provided in Appendix Table A1.

In addition, simple dynamics can be incorporated by including the employment state at time *t* as a regressor. Since the sample is restricted to employees in non-permanent jobs at *t*, this is equivalent to simply including a dummy variable identifying whether the respondent was employed on a fixed-term contract or on a casual basis. Finally, we test for whether the effect of training differs with employment type by interacting the participation in training variable with the lagged employment state.

With repeated measurements on the same individual, but observed at different points in time, this model can be estimated on a pooled data set, and the estimates obtained will be consistent provided there is no unobserved heterogeneity. But a key advantage of having repeated observations from the same individual is the ability to allow for individual time-invariant unobserved heterogeneity; e.g., in motivation or ability. Ignoring such unobserved heterogeneity will likely mean pooled models overestimate the impact of training on transitions to permanent employment because high-ability or highly motivated types, who are more likely to engage in training, will also be more likely to transition into permanent employment in any case.

Where the outcome is a continuous, dichotomous or count variable it is conventional to estimate models that allow for an individual-specific effect (or fixed effect), α_i, that captures all unobservable time-invariant characteristics (e.g., individual differences in motivation or ability). Until very recently, however, a fixed-effects estimator for models with a polychotomous discrete dependent variable was not available. Thus previous research has mostly either ignored the unobserved heterogeneity and estimated a pooled MNL model, or estimated an MNL model with random effects. The latter approach, for example, has been used by both Buddelmeyer and Wooden (2011) and Watson (2013). It, however, involves the assumption that α_i is independent of the observed covariates, which is widely recognised as unrealistic, and does not remove bias arising from the kind of correlated unobserved heterogeneity described above. D'Addio and Rosholm (2005) also make this assumption. There are thus strong grounds for preferring a MNL model with fixed effects, as proposed by Chamberlain (1980). We estimate such a model using the estimator recently developed by Pforr (2014).[5]

That said, although it seems entirely reasonable to make the assumption that the main source of omitted variables is time-invariant individual differences in, for example, motivation or ability, there may also be unobserved time-varying differences between individuals and, perhaps more interesting, unobserved differences at the firm level. For example, some firms employing workers on non-permanent contracts may use training as a screening device for permanent jobs and others may not (see Autor, 2001). The HILDA Survey, however, does not support adding firm-specific fixed effects, but we do include firm-level observable controls alongside time-varying individual observable controls (see Appendix Table A1).[6]

5. Results

Descriptive statistics

We begin our presentation of findings by first reporting, in Table 4, simple descriptive results from cross-tabulating annual average labour market transitions between times *t* and *t+1* by both employment type at *t* and participation in training in the previous 12 months (i.e., between *t-1* and *t*). For completeness, we report results for not only employees in casual and fixed-term contracts jobs, but also permanent employees.

Focussing first on men, we see that on average, 25.1% of casual employees that had participated in work-related training in the preceding year had progressed into permanent employment by the following year, while a further 7.0% had moved on to a fixed-term contract. The comparable figures for casual employees that were not exposed to training were lower – 21.7% and 5.1%. Further, casual employees without training were more likely to have exited employment by one year later – 15.7% compared with 10.9% of those that had received training. Among male employees on fixed-term contracts the patterns are less clear. Training is actually associated with both lower rates of progression into permanent employment (45.9% vs 48.9%) and lower rates of exit out of employment (3.3% vs 6.4%). There is thus some initial evidence here that, among men, training is associated with higher rates of progression out of casual employment into other forms of employment, but not out of fixed-term contracts.

Among female employees similar, but far smaller, differences are observed. Just over 21% of women in casual employment that had participated in training will have found permanent employment a year later, compared with just over 19% of those that had not participated in training. Much more significant is the lower rate of exit into non-employment – 9.8% of casual employees with training compared to 16.3% of those without. And as for men, there is no evidence here to suggest that training is associated with more rapid progression out of fixed-term contract employment into permanent employment.

MNL estimates: main results

We now consider the results from the MNL estimation. These are reported in Table 5. In the interest of brevity, we only present results on the coefficients of most interest; that is, those on the training participation variable, the employment status dummy and the interaction between the two. Results are reported for both men and women, and with and without controlling for unobserved heterogeneity (i.e., individual fixed effects). The coefficients are interpreted as the percentage change in the odds of ending up in destination *j* relative to that of ending up in the reference category (which in our set-up is casual employment), for a one unit change in the explanatory variable. Alternatively, by exponentiating the coefficients we can obtain the relative risk ratios. These are also reported (in square parentheses).

Focussing first on men, and consistent with the bivariate associations reported earlier, in a conventional MNL model that pools data but does not account for unobserved heterogeneity, we find evidence that work-related training during the previous year is associated with a higher probability of transiting into permanent employment. In our simplest model where we do not distinguish between casual and fixed-term workers, training increases the 'relative risk' of an employee in a non-permanent job moving into permanent employment by a factor of 1.49. In the pooled model where we do control for contract status at time t, training increases the relative risk of a non-permanent employee moving into permanent employment by a factor of 1.27. (The former estimate is larger because of the confounding effect of omitted contract status at time *t*.) This association is consistent with the positive and statistically significant associations reported by D'Addio and Rosholm (2005) for a sample of European countries and by Higuchi (2013) for Japan. We also find large differences between casual employees and fixed-term contract workers, with the former (the reference group in the estimation) much more likely to remain in casual employment (the base state) but also less likely to become jobless. The latter finding likely reflects the fact that not all fixed-term contracts are renewed when they reach their end date coupled with the fact that many casual jobs appear to be ongoing in practice, or at least long-lasting. The insignificant interaction term suggests that participation in training has no clear differential effects on the likelihood of fixed-term contract versus casual workers transiting into other labour market states, though it needs to be recognised that this is largely due to the relatively large standard errors attached to these estimated interaction terms. We also find a marginally significant negative association between training and the probability of being without a job next year.

Once we account for time-invariant unobserved heterogeneity, by controlling for individual-specific fixed effects, the training coefficient on transition into permanent employment shrinks in magnitude and is now a long way short of statistical significance; it is effectively zero. This is in clear contrast to D'Addio and Rosholm (2005), whose specification of unobserved heterogeneity assumes orthogonality with training (and all other observed covariates), and who unsurprisingly therefore find little difference in their training coefficient whether unobserved heterogeneity is included or not.[7] The negative association between training and exit into joblessness also becomes statistically insignificant, though in this case this is entirely the result of the increased imprecision of the estimation (which most likely reflects the much smaller effective sample size we are working with). We do still find, however, some weak evidence that training is associated with a higher likelihood of transiting into, or remaining in, a fixed-term contract position. Note also that the huge differences between casual employees and fixed-term contract workers are greatly reduced once individual characteristics are more fully accounted for.

Among women, Table 5 again suggests a positive association between training and transitions into permanent employment, but this disappears, even in the pooled model, once contract status at time *t* is controlled for. It is similarly zero in the fixed effects model. As for men we find big differences between casual and fixed-term workers, some of which survive the introduction of fixed effects, but no significant differences in training impacts between the two groups in either the pooled or fixed effects specifications. There are negative associations between training and transitions into other employment and non-employment in both pooled specifications but these fall in magnitude and become statistically insignificant in the fixed effects specification. The bottom line is that there is no evidence here of any impact of training on the likelihood of subsequent transition into permanent employment. Again, this is consistent with the findings of D'Addio and Rosholm (2005) for Europe, and of Higuchi (2013) for Japan.

Control variables are associated with transition probabilities largely as we would expect given the findings of existing studies, even in the fixed effects models. For example, the probability of transition to permanent employment is higher for both men and women with higher levels of education (e.g., D'Addio and Rosholm, 2005; Gagliarducci, 2007; Güell and Petrongolo, 2007; Buddelmeyer and Wooden, 2011; de Graaf-Zijl *et al*., 2011). We also include some control variables not commonly considered in the stepping stones literature (e.g., length of job tenure), for which a plausible *ex ante* case can be made. For both men and women we show that the probability of transition to permanent employment exhibits an inverted u-shaped relationship with job tenure; that is, increasing at first but eventually declining.[8]

Selectivity due to non-response

We considered the possibility that our estimates are biased due to endogenous sample response. There are 2189 observations that are not included in our estimation because they were not observed at *t+1*. Just over half of these missing cases (n=1177) were because the sample member either could not be located or refused to participate at the next interview, while most of the remainder (n=1001) were no longer in scope for the study (mainly because the respondent was not an original sample member and no longer co-resided with an original sample member).

To deal with this, we repeated the MNL estimation after incorporating an additional outcome state identifying sample members that did not respond to or participate in the survey at *t+1*. In the fixed effects MNL model, training participation is found to be inversely associated with the probability of being a non-respondent, and in the case of men the magnitude of the effect is quite large (relative risk ratio = 0.58), but nevertheless not quite statistically significant. Despite this, the estimates on the other coefficients are little affected. The estimated log odds of transitioning into permanent relative to casual employment among men, for example, remains highly insignificant and, at .036, is little different to that reported in Table 5 (.040). There is thus no evidence that non-random response has any noticeable effect on our results.

Additional sample restrictions

We also examined sensitivity to various additional sample restrictions. Excluding persons in full-time education leads to a loss of around 20% of the observations but makes no difference to the key results: a positive association between training and transition to permanent employment for men that is killed by the fixed effects, and no association for women with or without fixed effects. Restricting to prime-age workers (persons aged 25 to 54 years) leads to a loss of 50% of the sample for men and just over 40% of the sample for women, but again our key result remains unchanged: a positive association between training and transition to permanent employment (in this case for both men and women) that is eliminated after the inclusion of individual fixed effects. Excluding those on fixed-term contracts at time *t* (to concentrate only on casual employees) causes sample size to decline by close to 40% for men and around 30% for women, but again makes no noticeable difference to our key results.[9]

6. Discussion

This paper speaks to the intersection of two important international literatures: the literature on the impacts of training on job quality (usually wages), and the literature on the degree to which casual and other forms of non-standard employment are stepping stones on the way to permanent or standard employment. Specifically, we aim to quantify the role that work-related training plays in assisting workers progress from non-permanent to permanent forms of employment. There is sufficient *ex ante* ambiguity here, given the likely heterogeneity in the nature of training and the nature of workers across employment types, coupled with heterogeneity in the nature of transitions between employment types, that empirical evidence is crucial for even determining the sign of any such impact, let alone its magnitude. Despite this, existing evidence is thin on the ground. Few studies in the stepping-stones literature include a measure of training among their potential explanatory variables. Of those that do, no existing study satisfactorily addresses the issue of selection into training and how this biases estimates of the training impact on subsequent transitions. That is where this paper – the first to estimate training effects within a fixed-effects multinomial logit model of transitions out of non-permanent employment – makes its primary contribution.

For men we find that participation in work-related training is strongly positively correlated with the probability of moving from non-permanent to permanent employment. This in itself is not a unique result. We break new ground, however, by showing that this strong positive association is driven not by a causal impact of training on transitions, but by unobserved differences between workers in casual and temporary jobs who participate in training and those who do not, which are associated with the probability of transition into permanent employment (i.e., selection bias). Once these unobserved differences are washed out, on average participation in work-related training has no impact on the probability of subsequently moving into permanent employment. In other words, including individual fixed effects really matters.

This result is seemingly at odds with research that finds positive impacts of training (among workers more generally) on job quality couched in terms of wages, even after addressing the selection issue. There are a variety of potential explanations for this, but although our data offer tentative support for some of these, we are not able to distinguish between them. One potential explanation is that the work-related training engaged in by casual and temporary workers may be inferior in some key respect from work-related training engaged in by other workers. We provide evidence that workers in non-permanent employment who engage in training receive fewer hours of training than other workers, so even if training were identical in all other respects we might expect smaller impacts for non-permanent workers than for others. There is also a gender imbalance in training hours among non-permanent workers, especially workers on fixed-term contracts, so we might expect smaller impacts for women on such contracts.

Another potential explanation is that training that is firm-specific is unlikely to impact positively (and may even impact negatively) on transitions between firms. We argue that our training dummy captures both firm-specific and general training and we show that over half of transitions to permanent employment appear to occur between firms rather than within firms. If at least some of the training that takes place is unlikely to impact positively and may impact negatively on the single most important route for transitioning into permanent employment, an overall zero effect should perhaps come as no surprise.

In terms of policy implications, these explanations leave open the possibility that particular types of training intervention targeted at non-permanent workers – measures to increase training hours or other aspects of training quality and/or measures to boost general training among non-permanent workers, especially females – could help to boost transition rates into more secure employment, despite the null finding here.

In contrast, another potential explanation which does nothing to motivate such interventions, is that there may be no simple ‘job quality’ ordering between temporary, casual and permanent employment. This would be consistent with the lack of a clear, unambiguous ordering in self-reported job satisfaction among workers employed under different contractual arrangements across numerous studies and countries (see Wilkin, 2013). If so, the motivation for undertaking training may not predominantly be to move from casual or temporary employment into permanent employment. The weaker evidence even in the pooled model of a positive association between training and the likelihood of subsequent transition into permanent employment for women, more of whom may be secondary earners and may self-select into non-permanent employment to a greater extent than men (e.g., see Buddelmeyer *et al*., 2015), is arguably consistent with this.

Notes

[1] According to the OECD, which in turns draws on data from the European Labour Force Survey, permanent employment as share of all dependent employment among persons aged 25 to 54 years in Spain varied, over this period, from a low of 68.5% in 2006 to a high of 77.3% in 2013.

[2] The other pre-coded options available were: "to help you get started in your job"; "to maintain professional status and / or meet occupational standards"; "because of safety / health concerns"; and "other aims".

[3] Many studies have employed multinomial logit models within the context of a discrete time duration or hazard model (e.g., D'Addio and Roshom, 2005; Gagliarducci, 2005; Debels 2008; Gash, 2008; Leschke, 2009; de Graaf-Zijl *et al*., 2011). Although similar, our approach has two advantages, for our particular purposes, over this hazard model approach. First, our treatment of unobserved heterogeneity – individual fixed effects – allows for correlation between unobserved and observed factors associated with transitions, in contrast to the approach adopted by these earlier studies. Second, it is not necessary to drop left-censored spells of non-standard employment in our model – all historical (i.e., pre-survey) unobserved heterogeneity is washed out by the fixed effects – which allows us to retain a larger sample than would be the case were we to adopt a hazard model approach.

[4] For ease of estimation and presentation of results, and because of the relatively small number of cases that are observed in unemployment each wave, we have pooled the unemployed and not in the labour force categories.

[5] As in the more widely used fixed effects (or conditional) logit model, individuals with no changes in employment status during our sample period do not contribute to the likelihood and drop out of the estimation. More precisely, because our dependent variable is the type of transition, individuals with no variation in the type of transitions they make during the sample period are not used in the estimation. We lose around one third of all observations compared to the pooled MNL models for both men and women.

[6] There are numerous statistically significant differences in the observable characteristics, some of which are time-invariant (e.g., migrant status) and some time-varying (e.g., age, education level), of those who do and do not report receiving training in our sample. Training prevalence also varies by firm size, industry and location. These differences are controlled for in the model, but the implication is that there may also be unobserved differences between those who do and do not receive training. Some of these unobserved differences may be time-varying, and therefore not washed out by our individual fixed effects approach.

[7] We also find little difference between the pooled estimates and their random-effects equivalents.

[8] The full table of estimates including the controls is too unwieldy to include here, but these results are available from the authors upon request.

[9] All these additional results are available from the authors upon request.

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TABLE 1

Labour market state by sex and age group (% of population), 2003 to 2013: Australians aged 15-64

Sex / Labour market state	*Age group (years)*			
	15-24	*25-54*	*55-64*	*15-64*
Men				
Fixed-term contract employee	5.8	6.5	3.7	5.9
Casual employee	27.6	7.4	6.7	11.6
Permanent employee	28.5	57.2	33.1	47.1
Other employed	2.7	17.3	20.5	14.7
Unemployed	8.9	3.0	2.1	4.1
Not in the labour force	26.4	8.7	33.9	16.6
Sub-total	100.0	100.0	100.0	100.0
Women				
Fixed-term contract employee	4.5	6.6	3.3	5.6
Casual employee	34.0	11.9	7.5	15.7
Permanent employee	25.0	45.4	28.7	38.5
Other employed	1.2	8.7	9.0	7.3
Unemployed	7.4	2.9	1.3	3.6
Not in the labour force	27.9	24.5	50.3	29.4
Sub-total	100.0	100.0	100.0	100.0
Persons				
Fixed-term contract employee	5.2	6.5	3.5	5.8
Casual employee	30.8	9.6	7.1	13.6
Permanent employee	26.8	51.2	30.9	42.8
Other employed	2.0	13.0	14.7	11.0
Unemployed	8.2	3.0	1.7	3.8
Not in the labour force	27.1	16.7	42.2	23.0
Total	100.0	100.0	100.0	100.0

Note: Figures are cross-sectional population-weighted estimates for the pooled sample.

TABLE 2

Average year-to-year labour market transitions, 2003-2013 (%): Australians aged 15-64

Labour market state at t	*Labour force / employment status at t+1*					
	Fixed-term	*Casual*	*Permanent*	*Other employed*	*Unem-ployed*	*Not in the labour force*
Fixed-term	37.2	6.5	45.6	3.7	2.5	4.7
Casual	5.3	55.6	20.6	3.4	4.2	11.0
Permanent	5.3	4.3	83.5	2.2	1.3	3.5
Other employed	1.7	3.6	7.2	80.6	1.0	5.9
Unemployed	5.0	24.0	16.1	2.9	25.1	26.4
Not in the labour force	1.5	9.0	5.5	2.5	6.1	75.4

Notes: Figures are the average of weighted estimates of annual rates of transition (using paired longitudinal weights). Rows sum to 100.

TABLE 3

Participation in work-related training (in the previous 12 months) by employment type, sex and age group (% of employees), 2003 to 2013:

Australian employees aged 15-64

Sex / Employment type	*Age group (years)*			
	15-24	*25-54*	*55-64*	*15-64*
Men				
Fixed-term contract employee	45.0	33.3	28.0	35.2
Casual employee	14.5	15.5	12.9	14.8
Permanent employee	35.8	35.6	28.0	34.7
All employees	27.2	33.3	25.6	31.2
Women				
Fixed-term contract employee	41.9	42.2	31.1	41.1
Casual employee	18.3	21.0	17.6	19.5
Permanent employee	35.2	40.4	37.0	39.3
All employees	26.6	37.0	32.8	34.3
Persons				
Fixed-term contract employee	43.6	37.9	29.5	38.1
Casual employee	16.6	18.9	15.4	17.5
Permanent employee	35.6	37.7	32.2	36.8
All employees	26.9	35.0	29.0	32.7

Note: Figures are cross-sectional population-weighted estimates for the pooled sample.

TABLE 4

Average year-to-year employment transitions by employment type and training participation (%): Australian employees aged 15-64

	Labour market state at t+1					
Employment type / training status at t	*Fixed-term*	*Casual*	*Permanent*	*Other employed*	*Unem-ployed*	*Not in the labour force*
Men with training						
Fixed-term	42.9	5.1	45.9	2.8	1.4	1.9
Casual	7.0	52.1	25.1	4.9	4.1	6.8
Permanent	5.4	2.5	87.3	1.9	1.3	1.7
Men without training						
Fixed-term	33.3	5.9	48.9	5.5	2.9	3.5
Casual	5.1	53.3	21.7	4.2	5.5	10.2
Permanent	4.9	4.0	84.1	3.1	1.4	2.4
Women with training						
Fixed-term	41.2	6.6	42.4	1.6	2.4	5.7
Casual	5.9	61.5	21.3	1.6	2.8	7.0
Permanent	5.5	3.8	84.9	1.1	0.9	4.0
Women without training						
Fixed-term	35.1	7.7	44.0	3.4	2.6	7.2
Casual	4.7	56.6	19.2	3.1	3.5	12.8
Permanent	5.5	6.2	79.2	2.1	1.3	5.7

Notes: Figures are the average of weighted estimates of annual rates of transition (using paired longitudinal weights). Rows sum to 100.

TABLE 5

Training and the probability of transitioning out of non-standard employment: MNL coefficients (and standard errors and relative risk ratios)

	Labour market state at t+1			
Explanatory variable	*Permanent employee*	*Fixed-term contract*	*Other employed*	*Not employed*
Men: No fixed effects				
Work-related training between t-1 and t	.400***	.559***	.108	-.173
	(.079)	(.094)	(.161)	(.120)
	[1.49]	[1.75]	[1.11]	[.84]
N = 7581; Wald chi-squared = 2156.7 (p<.001); pseudo R-squared = .146				
Men: No fixed effects, fixed-term dummy and training interaction included				
Work-related training between t-1 and t	.237**	.332**	.111	-.234*
	(.099)	(.156)	(.204)	(.139)
	[1.27]	[1.39]	[1.11]	[.79]
Fixed-term contract at t	2.555***	3.409***	1.868***	1.750***
	(.140)	(.162)	(.205)	(.177)
	[12.87]	[30.23]	[6.47]	[5.75]
Work-related training x Fixed-term	-.256	-.258	-.480	-.266
	(.201)	(.238)	(.341)	(.293)
	[.77]	[.77]	[.62]	[.77]
N = 7581; Wald chi-squared = 2563.0 (p<.001); pseudo R-squared = .195				
Men: With fixed effects				
Work-related training between t-1 and t	.040	.409*	.005	-.248
	(.161)	(.240)	(.458)	(.230)
	[1.04]	[1.51]	[1.01]	[.78]
Fixed-term contract at t	.329	-.076	.965*	.594*
	(.200)	(.224)	(.518)	(.316)
	[1.39]	[.93]	[2.62]	[1.81]
Work-related training x Fixed-term	.115	-.216	-1.105	.061
	(.319)	(.360)	(.784)	(.478)
	[1.12]	[.81]	[.33]	[1.06]
N = 4772; Wald chi-squared = 558.8 (p<.001); pseudo R-squared = .135				
Women: No fixed effects				
Work-related training between t-1 and t	.223***	.227**	-.613***	-.211**
	(.066)	(.083)	(.177)	(.086)
	[1.25]	[1.25]	[.54]	[.81]
N = 10312; Wald chi-squared = 2280.6 (p<.001); pseudo R-squared = .123				
Women: No fixed effects, fixed-term dummy and training interaction included				
Work-related training between t-1 and t	-.007	.052	-.646***	-.378***
	(.080)	(.125)	(.222)	(.099)
	[.99]	[1.05]	[.52]	[.69]
Fixed-term contract at t	2.454***	3.225***	1.600***	1.498***
	(.112)	(.139)	(.204)	(.151)
	[11.63]	[25.15]	[4.95]	[4.47]
Work-related training x Fixed-term	.239	.056	-.007	.426*
	(.170)	(.206)	(.375)	(.225)

	[1.27]	[1.06]	[.99]	[1.53]
N = 10312; Wald chi-squared = 3152.0 (p<.001); pseudo R-squared = .176				
Women: With fixed effects				
Work-related training between t-1 and t	-.043	-.001	-.491	-.147
	(.127)	(.191)	(.394)	(.161)
	[.96]	[1.00]	[.61]	[.86]
Fixed-term contract at t	.800***	.053	.033	.364*
	(.175)	(.180)	(.463)	(.214)
	[2.23]	[1.05]	[1.03]	[1.44]
Work-related training x Fixed-term	.192	.249	.579	.239
	(.252)	(.277)	(.669)	(.324)
	[1.21]	[1.28]	[1.78]	[1.27]
N = 7005; Wald chi-squared = 812.6 (p<.001); pseudo R-squared = .132				

Notes: ***, ** and * denote statistical significance at the 99%, 95% and 90% confidence levels, respectively. Figures in curved parentheses are robust standard errors; figures in square parentheses are relative risk ratios. All models include all of the controls listed and described in Appendix Table A1, as well as nine survey wave dummies.

APPENDIX TABLE A1

Variable definitions and summary statistics for multivariate analysis (pooled sample of casual and fixed-term contract employees aged 15-64)

Variable	*Definition*	*Mean*	*Std. dev.*
Work-related training	= 1 if taken part in any structured work-related education or training schemes in past 12 months, and purpose of that training was to enhance skills or prepare for a future job or facilitate promotion; = 0 if otherwise.	.246	.430
Fixed-term contract	= 1 if employed on a fixed-term contract; = 0 if otherwise (i.e., employed on a casual basis).	.297	.457
Work-limiting health condition	= 1 if has any long-term health condition, impairment or disability that limits the type of work or the amount of work the person can do; = 0 if otherwise.	.094	.292
Non-work-limiting health condition	= 1 if has any long-term health condition, impairment or disability that does not limits the type of work or the amount of work the person can do; = 0 if otherwise.	.069	.254
Age			
15-24	=1 if aged between 15 and 24 on 30th June of previous financial year; = 0 if otherwise.	.384	.486
25-34	=1 if aged between 25 and 34; = 0 if otherwise.	.181	.385
35-44 [omitted]	=1 if aged between 35 and 44; = 0 if otherwise.	.177	.381
45-54	=1 if aged between 45 and 54; = 0 if otherwise.	.165	.371
55-64	=1 if aged between 55 and 64; = 0 if otherwise.	.094	.291
Partnered	= 1 if married or in a de facto relationship; = 0 if otherwise.	.496	.500
Employed partner	= 1 if has a partner who is employed (full- or part-time); = 0 otherwise.	.386	.487
Dependent children			
Child aged 0-4	= 1 if has any dependent children aged between 0 and 4; = 0 if otherwise.	.126	.332
Child aged 5-9	= 1 if has any dependent children aged between 5 and 9; = 0 if otherwise.	.143	.350
Child aged 10-14	= 1 if has any dependent children aged between 10 and 14; = 0 if otherwise.	.218	.413
ESB migrant	= 1 if born overseas in one of the main English-speaking countries (United Kingdom, New Zealand, Canada, USA, Ireland or South Africa); = 0 if otherwise.	.070	.255
NESB migrant	= 1 if born overseas but not in one of the main English-speaking countries; = 0 if otherwise.	.088	.284
Education			
Degree	= 1 if highest educational level is a bachelor (university) degree or higher; = 0 if otherwise.	.206	.405
Diploma	= 1 if highest educational level attained is an advanced diploma or diploma; = 0 if otherwise.	.067	.250
Certificate	= 1 if highest educational level attained is a Certificate Level III or IV (vocational) qualification; = 0 if otherwise.	.169	.375
Year 12	= 1 if highest educational level attained is completion of Year 12 of school; = 0 if otherwise.	.230	.421
Less than Year 12 [omitted]	= 1 if highest educational level attained is completion of Year 11 of school or lower; = 0 if otherwise.	.328	.469

Variable	*Definition*	*Mean*	*Std. dev.*
Household income (less own earnings)	Household gross income (excluding windfall income) for previous financial year less own gross financial year wages and salaries (A$000s), in constant prices (with missing values imputed).	75.08	81.76
Job tenure	Number of years employed with current employer.	3.25	5.20
Job tenure squared	Job tenure squared.	37.56	140.39
Occupational status	Occupational status of the main job, as determined by the Australian Socioeconomic Index 2006 (AUSIE06; McMillan *et al*., 2009). Scores range from 0 to 100.		
FT employment	= 1 if employed full-time (usual weekly working hours ≥ 35; = 0 if otherwise.	.396	.489
FT education	= 1 if in full-time education; = 0 if otherwise.	.215	.411
Union	= 1 if member of a trade union or employee association; = 0 if otherwise.	.161	.367
Workplace size			
<20 [Omitted]	= 1 if number of persons employed at place of work is fewer than 20; = 0 if otherwise.	.439	.496
20-199	= 1 if number of persons employed at place of work is between 20 and 199; = 0 if otherwise.	.405	.491
200+	= 1 if number of persons employed at place of work is 200 or more; = 0 if otherwise.	.155	.362
Location			
Sydney [Omitted]	= 1 if resides in Sydney; = 0 if otherwise.	.151	.358
Melbourne	= 1 if resides in Melbourne; = 0 if otherwise.	.163	.369
Brisbane	= 1 if resides in Brisbane; = 0 if otherwise.	.088	.282
Perth	= 1 if resides in Perth; = 0 if otherwise.	.070	.255
Adelaide	= 1 if resides in Adelaide; = 0 if otherwise.	.061	.239
Other major city	= 1 if resides in one of the other major cities, as determined by the Accessibility/Remoteness Index of Australia (mainly Canberra, Geelong, Gold Coast, Gosford, Newcastle, and Wollongong); = 0 if otherwise.	.086	.279
Inner-regional	=1 if resides in an inner-regional area as determined by the Accessibility/Remoteness Index of Australia; = 0 if otherwise.	.248	.431
Outer regional / Remote	=1 if resides in an outer-regional or remote area as determined by the Accessibility/Remoteness Index of Australia; = 0 if otherwise.	.135	.342
Industry			
Agriculture [omitted]	= 1 if industry of main job is in industry division, Agriculture, forestry and fishing, as determined by the Australian and New Zealand Standard Industrial Classification (ANZSIC) 2006; = if 0 if otherwise.	.025	.155
Mining	= 1 if industry of main job is in industry division, Mining, as determined by ANZSIC; = if 0 if otherwise.	.012	.110
Manufacturing	= 1 if industry of main job is in industry division, Manufacturing, as determined by ANZSIC; = if 0 if otherwise.	.062	.242
Utilities	= 1 if industry of main job is in industry division, Electricity, gas, water and waste services, as determined by ANZSIC; = if 0 if otherwise.	.007	.084
Construction	= 1 if industry of main job is in industry division, Construction, as determined by ANZSIC; = if 0 if otherwise.	.052	.221
Wholesale trade	= 1 if industry of main job is in industry division, Wholesale trade, as determined by ANZSIC; = if 0 if otherwise.	.021	.143

Variable	*Definition*	*Mean*	*Std. dev.*
Retail trade	= 1 if industry of main job is in industry division, Retail trade, as determined by ANZSIC; = if 0 if otherwise.	.166	.372
Accommodation & food	= 1 if industry of main job is in industry division, Accommodation and food services, as determined by ANZSIC; = if 0 if otherwise.	.147	.353
Transport	= 1 if industry of main job is in industry division, Transport, postal and warehousing, as determined by ANZSIC; = if 0 if otherwise.	.034	.182
Communication	= 1 if industry of main job is in industry division, Information media and telecommunications, as determined by ANZSIC; = if 0 if otherwise	.021	.142
Finance	= 1 if industry of main job is in industry division, Financial and insurance services, as determined by ANZSIC; = if 0 if otherwise.	.016	.127
Rental & real estate	= 1 if industry of main job is in industry division, Rental, hiring and real estate services, as determined by ANZSIC; = if 0 if otherwise.	.011	.105
Professional services	= 1 if industry of main job is in industry division, Professional, scientific and technical services, as determined by ANZSIC; = if 0 if otherwise.	.047	.211
Administrative services	= 1 if industry of main job is in industry division, Administrative and support services, as determined by ANZSIC; = if 0 if otherwise.	.032	.177
Public administration	= 1 if industry of main job is in industry division, Public administration and safety, as determined by ANZSIC; = if 0 if otherwise.	.047	.211
Education	= 1 if industry of main job is in industry division, Education and training, as determined by ANZSIC; = if 0 if otherwise.	.121	.326
Health	= 1 if industry of main job is in industry division, Health care and social assistance, as determined by ANZSIC; = if 0 if otherwise.	.123	.329
Arts & recreation	= 1 if industry of main job is in industry division, Arts and recreation services, as determined by ANZSIC; = if 0 if otherwise.	.028	.165
Other services	= 1 if industry of main job is in industry division, Other services, as determined by ANZSIC; = if 0 if otherwise.	.029	.168

Note: All models also include nine survey wave (i.e., year) dummies.